Utility of Sonic Tracking Technology to Evaluate the Effects of Environmental and Water Management Practices on Juvenile Chinook Salmon and Steelhead Migration Characteristics in the Interior Delta

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0263: Utility of Sonic Tracking Technology to Evaluate the Effects of Environmental and Water Management Practices on Juvenile Chinook Salmon and Steelhead Migration Characteristics in the Interior Delta

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Goal is to conduct ambitious and geographically extensive biotelemetry survey of migration and habitat use patterns of 160 Chinook (hatchery and wild) and 160 steelhead (hatchery only) juveniles over a two-year period. 51 Vemco receivers (30 already possessed by state) will be placed at critical pathways and intersection points in Mokelumne River, interior Delta, south Delta, Lower San Joaquin R., and confluence of Sacramento-San Joaquin Rivers. Mobile tracking will be restricted to the Mokelumne River and interior Delta. Patterns of juvenile transit and residency through the complex interior Delta system would be matched with flow (3 D acoustic system), temperature, salinity, and dissolved oxygen as well as patterns of water management. With some assumptions, survival of tagged fish can also be tracked between biotelemetry receivers. New miniaturization in acoustic tags will permit juvenile Chinook (100-150 mm) and steelhead (>150 mm) to be tracked for periods of 4 and 12 weeks, respectively. A unique element is the opportunity to include wild juvenile Chinook salmon (Mokelumne R) and compare their migrations with those of hatchery releases. An applied aspect is an evaluation of how migration routes are more likely to contribute to entrainment at SWP and CVP export pumping stations.

Evaluation: Above Average−Adequate

Additional Comments:

Strong justification was given for this type of study and its relevancy in linking essential juvenile salmon rearing and migration corridors with factors related to watershed management. PI s showed considerable attention to technical aspects of study including efficacy of tagging, placement of acoustic receivers, availability of fish for release, and collection of relevant environmental data. Team has solid experience with biotelemetry and would supply majority of acoustic receivers. Weaknesses included lack of statistical considerations on type of data that would result from the study (i.e., autocorrelated responses of many recaptures from relatively few individual fish), relatively short duration of acoustic tags (30 d. for Chinook), and a possible mismatch between the numerous locations that fish may move (e.g., 51 receivers) v. the relatively low no. of fish released each year (80 of each species). Research is risky, but is likely to ultimately pay off in a big way. This is a technology that can be very effectively deployed in the Interior Delta. As one reviewer, stated, "This proposal has the makings for a great research project." The PI s showed a sophisticated understanding of how juveniles may utilize the Interior Delta, which seemed inadequately matched by short duration tags and limited number of released fish. Also, not described but important is milling behaviors, which cause multiple recaptures at single receivers – how will these be interpreted? Arguably, the technology is limited and can be used to advantage, perhaps, to address a less ambitious set of hypotheses and objectives as laid out in the proposal. Increased involvement of statistician or population ecologist at start would seem justified given high cost of project. All reviewers thought the underlying approach – intercept biotelemetry – had strong application in the evaluation of watershed−level impacts on salmon rearing habitat, and viewed the PI s capable to undertake the study. One reviewer indicated such a project could not be eased into – it required full implementation; another reviewer suggested that an initial pilot study was needed to test fundamental assumptions

related to the effects of tagging on survival and behavior. Two reviewers noted the possible mismatch between the numerous potential fates of juveniles, v. the relatively few fish tagged and short duration of the biotelemetry possible for Chinook juveniles (30 d). The third reviewer was very critical on how the design could address the numerous hypotheses erected for migration patterns of juveniles through the interior delta, evaluation of tagging effects, lack of adequate literature review on hatchery v. wild salmon behaviors, and lack of adequate statistical considerations.

Goal is to conduct ambitious and geographically extensive biotelemetry survey of migration and habitat use patterns of 160 Chinook (hatchery and wild) and 160 steelhead (hatchery only) juveniles over a two−year period. 51 Vemco receivers (30 already possessed by state) will be placed at critical pathways and intersection points in Mokelumne River, interior Delta, south Delta, Lower San Joaquin R., and confluence of Sacramento−San Joaquin Rivers. Mobile tracking will be restricted to the Mokelumne River and interior Delta. Patterns of juvenile transit and residency through the complex interior Delta system would be matched with flow (3 D acoustic system), temperature, salinity, and dissolved oxygen as well as patterns of water management. With some assumptions, survival of tagged fish can also be tracked between biotelemetry receivers. New miniaturization in acoustic tags will permit juvenile Chinook (100−150 mm) and steelhead (>150 mm) to be tracked for periods of 4 and 12 weeks, respectively. A unique element is the opportunity to include wild juvenile Chinook salmon (Mokelumne R) and compare their migrations with those of hatchery releases. An applied aspect is an evaluation of how migration routes are more likely to contribute to entrainment at SWP and CVP export pumping stations.
Evaluation: Above Average−Adequate

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Utility of sonic tracking technology to evaluate the effects of environmental and water management practices on juvenile Chinook salmon and steelhead migration characteristics in the interior Delta

The primary reviewer ranked this proposal as above average. Reviewers felt the proposal showed good attention to technical data and the research team had solid demonstrated experience in the field. The project has promise for providing valuable information on juvenile salmon movement throughout the delta, although the proposal was somewhat ambitious it its objectives. It also offers potential valuable information on differentiating differences in behavior of hatchery vs. wild Chinook salmon. The panel expressed concerns regarding the low number of fish being tagged, the short duration of the tag life, and a general lack of adequate literature review. The need for greater involvement of biostatisticians and population biologists to manage and analyze the data limited the value of the proposal. There were significant concerns regarding how the data will be dealt with and what the end products will be with regard to contributions to peer reviewed literature. For these reasons the panel rated the proposal adequate.

Final Ranking: Adequate

Technical Review #1

proposal title: Utility of Sonic Tracking Technology to Evaluate the Effects of Environmental and Water Management Practices on Juvenile Chinook Salmon and Steelhead Migration Characteristics in the Interior Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goal in the proposal is to improve understanding of factors influencing juvenile salmonid migration using new tracking technology. The work has important implications for scientists and resource managers attempting to improve management strategies for species of concern in the San Francisco–San Joaquin Delta. Unfortunately, the proposal goal is vague in that it doesn't explicitly state which factors will be studied with regard to migration. Additionally, there are study objectives which don't appear to be closely related to the overall goal (e.g. objective 4, documenting high mortality). The hypotheses are clearly stated, but are potentially too numerous. For instance, hypothesis 1 is that the survival and behavior of juvenile salmonids is unaffected by acoustic tags or the tagging process. That hypothesis alone could be divided into four separate tests (Ho1: Survival is unaffected by acoustic tags. Ho2: Survival is unaffected by the tagging process. Ho3: Behavior is unaffected by acoustic tags. Ho4: Behavior is unaffected by the tagging process). It is also questionable whether some of the hypotheses are testable. Under the current design, it may be advisable to eliminate the less essential hypotheses and focus on two or three that would be testable and provide the most information regarding migration. Another important point to note is that several**

	assumptions on which this study is based are highly questionable. If these assumptions are not true, then the entire foundation of the study is lacking and study results could be meaningless. The proposal would have been greatly improved if the authors had cited recent literature that indicates that there are no behavioral differences between hatchery and wild salmon, no differences between behavior and predation rates of tagged and untagged fish. There is a large body of literature that addresses the differences between hatchery and wild fish, and while there may not be a definitive answer on behavioral differences, the proposal could have at least addressed why assumptions were made disregarding any differences. In addition, the authors note that this will be the first comparison of migration characteristics and survival for natural and hatchery Chinook salmon, which is not true (DeVries et al. 2004, Clements, Shaun et al. 2004, Ryan, Brad A. et al. 2003, etc.). Reviewing the existing literature, drawing from recent conclusions, and expanding into unknown areas of interest would also have improved the proposal.
Rating	**fair**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This study is justified because improving existing knowledge about the use of tracking technology to understand migration behavior would be extremely useful for resource managers in the Delta. In addition, if the study also led to improved understanding of migration behavior, it would be welcome information for scientists and resource managers alike. Using new technology for tracking juveniles is also a compelling idea and could produce interesting results. However, while the topic is**

	extremely exciting, the study design does not appear to be able to be capable of producing robust results that would greatly increase our understanding of acoustic technology nor of juvenile migration. The proposal indicates that the conceptual model is decidedly unclear because of lacking information regarding migration behavior for salmonids in the Delta. However, the proposal clearly reviews current understanding of juvenile migration in the Delta and how the conceptual underpinnings will be included in the study design. The project objective involves first evaluating the potential use of new acoustic tracking technology for understanding migration patterns of juveniles. It would make more sense then, if this was a pilot project rather than a full scale research project. The remainder of the project depends on the successful use of tracking technology to later make conclusions. Therefore, this study would be better suited to a pilot project that assesses the use of acoustic tags for juveniles rather than the current design including a full-scale research project based on the first part of the study.
Rating	**fair**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The entire study is not outlined in much detail and lacks key design elements and statistical analyses. This makes it difficult to assess whether it will be possible to draw conclusions from the study. The following is a list of potential pitfalls within the tasks outlined in the proposal. · Task 2.1 – 1) Fails to include a true control, handling fish without implementing tags is a treatment. A control fish group, that is not handled at all, would be helpful in**

assessing if there were adverse effects from handling which is a part of the tagging procedure. 2) It doesn't appear that behavior will be monitored. Behavior experiments generally involve careful supervision, and repeated intensive monitoring, this is not outlined in the proposal. A clearer description of behavioral monitoring would be necessary if they actually propose to understand behavioral differences. · Task 2.2 – 1) It is unclear why the number of fish for tag and release was not decided upon. Without a number of tagged fish in the design, it is difficult to assess whether the data collected will be statistically relevant. 2)This proposal cannot compare "behavior," nor , "preferences," with this design, rather it can compare the route and rate of two groups of fish. 3) Saying that they will use descriptive statistics is extremely vague. · Objective 3 – It is not possible to evaluate the effects of flow in migration characteristic with this project design. There are several environmental conditions that could affect migration route choice, and this study does not control for any of those confounding elements. Additionally, it is likely there will be covariance among the variables. Moreover, with this design, there can only be correlations drawn from the data. There is also no mention of whether spatial autocorrelation will be a problem (which could be possible, but again the design is vague). There is also no mention of how the statistics will deal with covariance. · 3.1 Pos hoc designing is generally a bad idea for a project of this size and with such high monetary demands. · 3.3 1) There is no mention of how they will assess the effectiveness of mobile monitoring other than that they need, "meaningful information gathering and reasonable costs". 2) Confounding effects of multiple variables make it difficult to imagine that they will be able to draw conclusions about the environmental and operation conditions that affect migration patterns with the current design. Again, there is no mention of how data will be analyzed. · Objective 4 – Statistics were not outlined, rather they were listed

	as a suite of potential statistics, some of which seem less than ideal or inappropriate for this study (T−test and ANOVA are best suited for experiments with more control than outlined in this proposal). In addition, it is difficult to say whether a least square regression model will reflect real migration patterns with the study outline, especially with confounding factors. The proposal authors may want to investigate alternative statistics such as PCA (or other techniques to reduce the amount of variables being considered), and potentially another multivariate approach that will build a predictive model (perhaps also using something like Akaike Information Criteria (AIC) to compare alternative models). · Objective 5 – It does not seem possible to identify causes of mortality with this design. The lack of fish being picked up by tracking receptors could be due to a number of reasons (i.e. out of range, faulty equipment, loss of tag, as well as mortality due to several factors). Without finding the fish it is strange to say that it will be possible to know the cause of death. Also, the proposal states that it will be possible to know the probability of tag loss, from results from objective 2. However, it was not clear in Objective 2, how they would calculate this probability. · 5.3 1) Again, it would be hard to know with all the potential variables to distinguish which were affecting mortality events. 2) No mention of statistics again. · A final note, it is hard to believe that the Mokelumne river is the only source of the appropriate size range of salmonids (page 20).
Rating	**poor**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is not documented with much detail, rather it attempts to address a wide variety of study**

	areas in limited detail. The ultimate result is that the proposal lacks a clear description of many of the key design elements (number of fish to be tagged, releasing protocol are two examples) and a plan for statistical analysis following data collection. With regard to technical feasibility, it is possible that the acoustic technology could work, as several researchers are currently using similar technology, but per their description this has never been tried for juvenile fish of this size.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	There are no controls in the majority of the studies proposed, and in some experiments the control is lacking (objective 2, page 9). Thus the majority of data will lead to correlative results and the proposal does a poor job of explaining the statistical techniques they will use to constructively analyze the data collected.
Rating	fair

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The products from the proposed work include progress, fiscal and annual reports as well as at least one presentation at a conference or workshop. These products are likely to be extremely valuable, if the data is reliable and the analyses are robust, because increased understanding of juvenile migration could

	improve resource management decision-making processes. The proposal mentions that the data will be shared with other acoustic tracking programs and that collaboration will benefit all projects. However these benefits from sharing data are not clearly defined. The proposal notes that distribution of annual reports will be made to agency participants and interested parties. The proposal could have been improved by dispersing information via the internet as well. There are several public agency websites where data and annual reports could be made accessible to the public and interested parties.
Rating	fair

Additional Comments

Comments	In general, this proposal would be greatly improved by limiting the number of hypotheses to a few clearly testable ones, clarifying the study design in greater detail, and planning the statistical analysis.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team is well qualified to manage, implement, and conduct field monitoring of the proposed work. The team is composed of a diverse group of individuals, with varying skills. Most team members have an abundance of salmonid monitoring and assessment experience. The combination of skills is appropriate to implement and manage the project successfully, and the team's past experience indicates that they will. However, it is important to keep in mind comments mentioned in previous sections regarding final data analysis and interpretation. The lead investigator for this

	project, as well as the two Senior consultants have never published in peer−reviewed journals. While one would hope the lead investigator and senior consultants have the skills to adequately analyze the data (given their past experience with fisheries research), none of the statistical procedures were elaborated on in detail and perhaps the most appropriate statistical analysis tools were not even mentioned. This makes it difficult to assess whether the team has the skills to do the analysis successfully.
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget asks for $613, 054 for a two−year project. This may seem like a large amount for a two year project, but could be due to the nature of conducting a multifaceted, large−scale project, and involving consultants as well as public and private agencies. Three items to note in the budget include the following: · 21 VR2 recievers and tags for fish were not included in the budget. It was unclear where the money for this would come from and if it was guaranteed. · The proposal indicates that a biostatistician will review the potential analyses of the data (tasks 4.2, 5.2), but there is no indication of who this biostatistician will be or whether this will cost additional funds (also, should be noted that consulting a biostatistician would be more effective before beginning the work, rather than after data collection). · The proposal requests $25, 170 to participate in a workshop. This amount seems potentially excessive for participation in only one workshop (Task 1.4).**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**This proposal has the makings for a great research project, because it outlines a compelling and interesting project with the potential to advance our knowledge of juvenile migration. However, the key to a sound research proposal is that it describes testable hypotheses and elaborates how to test the hypotheses. This proposal failed to adequately do both. Some of the hypotheses were arguably not testable under the current design, and the statistical analysis was lacking. The proposal would have been greatly improved by carefully outlining the design and statistical analysis, enhancing the potential to produce reliable data and robust results.**
Rating	**fair**

Technical Review #2

proposal title: Utility of Sonic Tracking Technology to Evaluate the Effects of Environmental and Water Management Practices on Juvenile Chinook Salmon and Steelhead Migration Characteristics in the Interior Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The objectives are clearly written and consistent with the larger goal of increased understanding of factors influencing juvenile salmonid migration through the Delta. Several underlying hypotheses related to the experimental use of sonic tagging to follow juvenile fish migration are also in support of the goal.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The proposed project is fully justified as the use of the newest technology should allow for the first time the tracking of juvenile salmonids in a manner not previously possible. Consequently little is known of juvenile migration behavior and the state of knowledge should be significantly increased by this study.**
Rating	

	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The investigators have presented a detailed and logical study design that should demonstrate the utility of sonic tags for tracking juvenile salmonids. The study design also provides much promise for increasing the knowldge base of juvenile salmonid migrations and factors influencing.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Previous experiences of the investigators with similar tagging studies along with the well laid out study design indicate that the project is very feasible.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Initial monitoring is through distribution of fixed recording sensors distributed throughout the inter Delta. These data will be analyzed through time and space following the first**

	year. This is intended to lead to hypotheses of movement in the interior Delta and the second year monitoring is to be "adapted" to examine possible factors influencing behavior and movement.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The many progress reports, presentations and publications should document the utility of sonic tagging for tracking juvenile salmonids. Secondary products resulting from observations should advance the knowledge base and factors influencing juvenile salmonid migration behavior.
Rating	excellent

Additional Comments

Comments	The initial products will be related to the utility of sonic tagging of small juvenile salmonid for tracking seasonal movement patterns. The following products will be more toward increased knowledge of juvenile salmonid migration routes, patterns and posible influencing factors (assuming the utility is first demonstrated).

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The investigators apppear well qualified, have extensive experience with similar studies on larger

	fishes and have published previous work.
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is very detailed (almost overdone) and should be quite adequate to complete the proposed study.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is a very good proposal, well documented and supported by well qualified investigators. This is one of the better proposals that I have reviewed for the CALFED program.**
Rating	**excellent**

Technical Review #3

proposal title: Utility of Sonic Tracking Technology to Evaluate the Effects of Environmental and Water Management Practices on Juvenile Chinook Salmon and Steelhead Migration Characteristics in the Interior Delta

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals, objectives and hypotheses are very clearly stated and are internally consistent. They specifically addresses several of the Science PSP goals, as outlined on page 1 of the proposal. This work should provide a thorough monitoring of the migration patterns of 160 juvenile salmon. The direct comparison of migratory patterns of natural and hatchery Chinook is a particularly worthy and useful goal. Evaluation of the timeliness and importance of this is beyond my expertise.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The existing knowledge related to migratory patterns in this system is beyond my expertise. The conceptual model considered includes hypotheses from three models that predict that juvenile salmon survival is influenced by (1st model) positive net downstream flow, (2nd) flow in the lower San Joaquin determines direction and survival and (3rd) migratory behavior is**

	largely driven by environmental stimuli. The conceptual model of this study is that all of these things may influence migration and the study will attempt to related monitored migrations of the 160 juveniles with these variables, explaining the basis for this work. This is certainly a full-scale implementation of the project. Because of the method used, anything less than a full scale would likely result in unsatisfactory data. If fewer stationary receivers are used the fish are likely to migrate out of the array of detectors, compromising the results of this study.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The proposal lacks statistical justification for the choice of the number of fish that will be tagged. Other than this, the overall approach is well designed and appropriate for meeting the objectives of the project. The project is large in scale, owing to the nature of the question. It's fundamentally difficult and involved to track migration patterns over this spatial scale. This is a feasible way to do this and the results will add to a base of knowledge in the form of information on migration patterns of a critical species that is really fundamental to the stated interests of CALFED. This information would certainly be useful to decision makers.** **The first objective is to address the effects of the tags themselves and this is a well conceived plan, although I am surprised that similar studies comparing the behavior of tagged and not tagged fish in a hatchery environment have not already been done. The**

control in these experiments are fish that are handled as though there is a surgery, but no incision is made or tag inserted. Wouldn't a better control to the tagged fish be fish that were not handled and anesthetized since the objective is to show that the tagged fish behave as natural fish, not as natural fish that have undergone the stress of being handled and anesthetized for a surgical procedure?

The task directed at comparing behavior of natural and hatchery Chinook is well conceived and very relevant to a fundamental assumption in the methods of this and other studies.

A significant portion of the budget is for receivers of which 21 will be acquired by this proposal and 30 are currently owned by CDFG.

How many fish will be tagged? In the executive summary it says 160 juveniles over the two years, but this is not discussed in the body of the proposal. Given the large number of potential routes and confounding environmental and operational conditions, it is not clear to me that the proposed sample size of large expense is sufficient to resolve these questions with significance. Preliminary statistical analysis justifying the sample size would help, particularly knowing that the cost in labor and transmitter ($300) is significant for the fish. Contrarily, all of the cost for new receivers and installation (190K) the project would be wasted if 160 is not adequate sampling. The discussion of statistical analysis is a vague and generic paragraph used in this and many other proposals, showing the lack of careful experimental design.

Is a duration of 30 days long enough to determine migration patterns in the juveniles? The battery life is the limiting factor in the duration of these experiments. Adjustment of the period between emitted pulses leads to a trade–off between detectability and

	duration of the experiment. The authors arrive at choosing a period that will lead to the transmitter batteries lasting 30 days for the juveniles, but if this has a biological basis or if this is sufficient to capture adequate information on the migrations, this is not explained in the proposal.
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The method is certainly feasible. It is a large−scale implementation of a proven technology to observe a limited number of fish for a 2 year period.** **The approach is fully documented if Appendix A explaining the operation of Sonic transmitters is included. The likelihood of success as measured by accomplishment of all objectives to some extent is likely. The scale of the project is very consistent with a objectives and is, in short, rather large. The scale and methods are all within the grasp of the authors.** **At least two of the team members (Stein, Vincik) have experience with telemetry and one of them with the particular Vemco brand of ultrasonic transmitters proposed to be used, although it seems that neither has implanted them into juvenile fish.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The study is essentially monitoring of migration patterns and then correlation of this data to other factors. This is the interpretation portion of the proposal and is significant. The proposal reads as though some interpretation of the data will be done within the first of the two-year contract period. Possible modifications to the experiment may be made for the second year of the contract. The data on migratory patterns of these 160 fish would certainly be available for interpreation after the contract. The plans for interpreting the monitoring data seems to be appropriately designed, although are still vague within this proposal.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**There are certainly products of likely value from this project regarding migration patterns of a valuable species. The interpretation of these in relation to environmental and usage variables. Interpretive and interpretable outcomes are likely, see above answer.**
Rating	**very good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**I am certain that the funding agency is familiar with California Urban Water Agencies, California Department of Fish and Game and SP Cramer and Associates and is better able to judge their track record than I am.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**This is a large budget. I feel that it's reasonable for the work proposed, it's just a lot of work. Whether the knowledge gained regarding detailed and fairly thorough migration patterns for 160 fish justifies the expense is beyond my expertise.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposal addresses a useful and specific question related to migratory patterns a valuable species. I believe that it will be successful and lend some useful information, although possibly not to the complete satisfaction of all objectives, of which some are somewhat vague. One disappointment in the method used by this proposal is the limited duration of observation of each fish, which is only limited by the battery life of available sonic transmitters. It is also not clear to me if the sample size proposed to be collected is really sufficient to address the numerous possible relationships between migratory patterns and physical and environmental variables described here.**
Rating	**very good**